

8 cm. from the primary. *Reverse the wires in the secondary circuit, reverse the wires in the primary circuit, how you please, the mercury always moves towards the point of the capillary.*

8. Shouting or singing (excepting the above-mentioned note) produces no visible effect under the conditions mentioned in Experiments 5, 6, and 7.

9. If the secondary coil be now moved close up, so as to cover as completely as possible the primary, talking to the telephone with the ordinary voice, *i.e.* with moderate strength and at any pitch, produces a definite movement of the mercury column for each word, some sounds of course giving more movement than others, *but the movement is always towards the end of the capillary.* Singing the note mentioned in Experiments 5, 6, and 7 loudly, produces a movement too large to be measured with the electrometer.

Reversing the poles of the magnet in the telephone does not alter the results of Experiments 5, 6, 7, and 9.

On mentioning the above results to Dr. Burdon Sanderson, he suggested that the apparently anomalous behaviour of the electrometer might be accounted for, by supposing that the mercury moved *quicker* when a current passed towards the point of the capillary than when it flowed in the opposite direction; so that if a succession of rapidly alternating currents be passed through the instrument, the mercury will always move towards the point of the capillary, the movement away from the point being masked by the sluggishness of the instrument in that direction. That this explanation is the correct one is proved by the following experiment:—The current from two Grove's cells is sent through a metal reed vibrating 100 times a second, the contact being made and broken at each vibration, the primary wire of a Du Bois Reymond's induction-coil is also included in the circuit; on connecting the electrometer with the secondary coil placed at an appropriate distance the mercury always moves to the point of the tube whatever be the direction of the current.

F. J. M. PAGE

Physiological Laboratory, University College,
London, February 2

NOTE.—On February 4 Prof. Graham Bell kindly placed at my disposal a telephone much more powerful than any of those I had previously used. On speaking to this instrument, the electrometer being in the circuit, movements of the mercury column as considerable as those in Experiment 9 were observed.—F. J. M. P.

CHEMISTRY AND ALGEBRA

IT may not be wholly without interest to some of the readers of NATURE to be made acquainted with an analogy that has recently forcibly impressed me between branches of human knowledge apparently so dissimilar as modern chemistry and modern algebra. I have found it of great utility in explaining to non-mathematicians the nature of the investigations which algebraists are at present busily at work upon to make out the so-called *Grundformen* or irreducible forms appurtenant to binary quantics taken singly or in systems, and I have also found that it may be used as an instrument of investigation in purely algebraical inquiries. So much is this the case that I hardly ever take up Dr. Frankland's exceedingly valuable "Notes for Chemical Students," which are drawn up exclusively on the basis of Kekulé's exquisite conception of *valence*, without deriving suggestions for new researches in the theory of algebraical forms. I will confine myself to a statement of the grounds of the analogy, referring those who may feel an interest in the subject and are desirous for further information about it to a memoir which I have written upon it for the new *American Journal of Pure and Applied Mathematics*, the first number of which will appear early in February.

The analogy is between atoms and *binary* quantics exclusively.

I compare every binary quantic with a chemical atom. The number of factors (or rays, as they may be regarded by an obvious geometrical interpretation) in a binary quantic is the analogue of the number of *bonds*, or the *valence*, as it is termed, of a chemical atom.

Thus a linear form may be regarded as a monad atom, a quadratic form as a duad, a cubic form as a triad, and so on.

An invariant of a system of binary quantics of various degrees is the analogue of a chemical substance composed of atoms of corresponding *valences*. The order of such invariant in each set of coefficients is the same as the number of atoms of the corresponding *valence* in the chemical compound.

A co-variant is the analogue of an (organic or inorganic) compound radical. The orders in the several sets of coefficients corresponding, as for invariants, to the respective valences of the atoms, the free valence of the compound radical then becomes identical with the degree of the co-variant in the variables.

The weight of an invariant is identical with the number of the bonds in the chemicograph of the analogous chemical substance, and the weight of the leading term (or basic differentiant) of a co-variant is the same as the number of bonds in the chemicograph of the analogous compound radical. Every invariant and covariant thus becomes expressible by a *graph* precisely identical with a Kekuléan diagram or chemicograph. But not every chemicograph is an algebraical one. I show that by an application of the algebraical law of reciprocity every algebraical graph of a given invariant will represent the constitution in terms of the roots of a quantic of a type reciprocal to that of the given invariant of an invariant belonging to that reciprocal type. I give a rule for the geometrical multiplication of graphs, *i.e.* for constructing a *graph* to the product of in- or co-variants whose separate graphs are given. I have also ventured upon a hypothesis which, whilst in nowise interfering with existing chemicographical constructions, accounts for the seeming anomaly of the isolated existence as "monad molecules" of mercury, zinc, and arsenic—and gives a rational explanation of the "mutual saturation of bonds."

I have thus been led to see more clearly than ever I did before the existence of a common ground to the new mechanism, the new chemistry, and the new algebra. Underlying all these is the theory of pure colligation, which applies undistinguishably to the three great theories, all initiated within the last third of a century or thereabouts by Eisenstein, Kekulé, and Peaucellier.

Baltimore, January 1

J. J. SYLVESTER

PALMEN ON THE MORPHOLOGY OF THE TRACHEAL SYSTEM

DR. PALMEN, of Helsingfors, has recently published an interesting memoir on the tracheal system of insects. He observes that although the gills of certain aquatic larvæ are attached to the skin very near to the points at which the spiracles open in the mature insects, and though spiracles and gills do not co-exist in the same segment, yet the point of attachment of the gills never exactly coincides with the position of the future spiracle. Moreover, he shows that even during the larval condition, although the spiracles are not open, the structure of the stigmatic duct is present, and indeed that it opens temporarily at each moult, to permit the inner tracheal membrane to be cast, after which it closes again. In fact, then, he urges, the gills and spiracles do not correspond exactly, either in number or in position, and there can therefore be between them no genetic connection. He concludes that the insects with open tracheæ are not derived from ancestors provided with gills,

but, on the contrary, that the possession of a closed tracheal system is a secondary condition, derived from ancestors provided with spiracles.

He adopts the view that the existing insects are derived from an ancestor, in which the larvæ resembled the existing genus *Campodea*, with a hemimetabolous metamorphosis, and an open tracheal system; and he dwells on the important fact that in *Campodea* each spiracle has an independent set of tracheæ. So also in the course of embryonal development, the tracheal systems rise separately, and then the anterior and posterior branches unite to form the lateral ducts.

In a still earlier stage he thinks it probable that the tracheæ resembled those of the curious genus *Peripatus*. He observes that the skin-glands of certain worms secrete not only fluid, but also gas (carbonic acid), and from this to an absorbing function would be a comparatively small step. He supposes, then, that the tracheæ are derived from the skin-glands of worms, passing firstly through the stage now represented by *Peripatus*, in which there are a number of tracheal tubes with numerous scattered openings; secondly, though one represented now by *Campodea* and certain myriapods, in which the spiracles are situated in pairs, and are connected with separate tracheal systems.

I. L.

ON THE EVOLUTION OF HEAT DURING MUSCULAR ACTION¹

PROF. A. FICK, of Würzburg, in continuing his researches on the source of muscular power, has obtained some new and exceedingly important results, of which the following is a condensed account:—

It is obviously an interesting question in the physiology of muscle what fraction of the work yielded by chemical action in muscular tissue can be employed in overcoming mechanical resistance? the remainder of the chemical work appearing, in all probability, as heat.

Many years ago Helmholtz calculated, from certain considerations, into which, however, there entered several hypothetical factors, that possibly one-fifth of the total work yielded by chemical force in the human body might be employed in muscular action, the remaining four-fifths appearing as sensible heat. From this it necessarily follows that a much larger proportion than one-fifth of the work yielded by chemical force in the muscle itself can be employed in overcoming mechanical resistance, inasmuch as it is assumed that a great part of the oxidation takes place in other tissues, where mechanical work is quite out of the question, and where heat alone can be the result.

If, however, thermodynamical experiments show that of the chemical work going on in the muscle only a small fraction, not much exceeding one-fifth, produces mechanical effect; then, supposing the coefficient of Helmholtz to be true, it would be proved that only minute quantities of combustible material are oxidised elsewhere than in the muscles. The author's experiments have been made with a view to answer the first of the above questions—what fraction of the chemical force eliminated in the muscle is used in mechanical work? Such experiments can, of course, with the present means of research, only be carried out upon the muscles of the frog. How far the results obtained are applicable to other classes of animals, is a distinct question.

Thus two magnitudes have to be determined in absolute measure, viz., the mechanical work performed by the muscle, and secondly, the amount of chemical work that the muscle has yielded during the action.

The amount of heat produced in the muscle was of course measured by multiplying the rise in temperature of the muscle by its capacity for heat. In the calculations the specific heat of muscle was taken as equal to that of water. It cannot be greater, and is probably not

¹ Ueber die Wärmeentwicklung bei der Muskelzuckung," in the *Archiv. f. Physiologie*, Band xvi.

much less, inasmuch as three-fourths of living muscle are water. The rise in temperature was measured by thermoelectrical means. The galvanometer used had no fixed magnet, and its constancy was proved to extend over many weeks, and even months. The thermopile had to be so arranged that it was as much as possible surrounded by the mass of muscle; its construction will be better understood after the preparation has been described. The gastrocnemius muscle, which is the favourite preparation in such experiments, was replaced by the masses of muscle which pass from the pelvis to the tibia on the inner side of each thigh, whilst the other muscles, with the sartorius and biceps, as well as both the thigh-bones, were removed. Then, on suspending the pelvis, the two prepared masses of muscle hung vertically downwards in intimate contact with each other, all the nerves belonging thereto being easily preserved. One end of the thermopile, with very flat and thin elements, was then placed in the fissure between the two masses of muscle, this arrangement being found by experience to be a perfectly trustworthy one.

A remark is necessary concerning the method of irritating the preparation. Some years ago the author had the opportunity of observing, in some unpublished experiments, that an electric current of sufficient strength to produce the most powerful contraction in a muscle, does not appreciably raise the temperature of the latter. Even with Heidenhain's exceedingly delicate thermopile there was scarcely any evidence of heat being produced in a dead muscle through which a current of twenty-four Daniell's elements was passing for several seconds; and even induction currents of immense strength produced no visible thermal effect. This fact is of great interest in myothermic experiments, as it is thus no longer necessary to impart the stimulus through the nerve, but simply to subject the muscle to direct electrical irritation.

In his experiments, the author has adopted preferentially the method of direct irritation, one of the two copper wires connected with the induction-coil being attached to the pelvis, and the other to the knee of the frog.

The mechanical work was measured by connecting the preparation with one arm of a lever to which a weight was attached, and, in some of the experiments, there were also two balanced weights placed upon the lever to increase its inertia, by which it was found that the work performed was very considerably increased.

The following is a summary of the chief results arrived at by these experiments:—

1. By the interposition of a thin thermopile between suitable masses of muscle, it is possible to determine with great accuracy the absolute amount of heat produced by their contraction.

2. The determination of the muscle-temperature is not interfered with by electrical currents, which, for the purpose of irritation, are passed through the muscle. Therefore direct electrical irritation of the muscle is permissible, and indeed far preferable, in myothermic researches.

3. To the fundamental law of Heidenhain, that a muscle contracting to its greatest extent evolves more heat the greater its initial tension, we may now add that, with equal initial tension, a muscle will evolve more heat if, by means of weights in equilibrium, greater tension be produced during the contraction.

4. A muscle overcoming a greater resistance, works not only with more activity but also with more economy than when occupied in a smaller effort.

5. In an energetic muscular contraction against as great a resistance as possible the eliminated chemical force is about four times as great as the mechanical work it performs. With a less resistance the chemical is a greater multiple of the mechanical force, and with no resistance at all it is obviously indefinitely greater.

6. The amount of heat produced by the eliminated chemical force in an energetic contraction of 1 grm. of